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RECENT REVIVALS OF DARWINISM[1]

By Dr. HENRY FAIRFIELD OSBORN

AMERICAN MUSEUM OF NATURAL HISTORY

BEFORE us are four recent volumes, the "Huxley Memorial Lectures," "The Causes of Evolution," by J. B. S. Haldane, "Problems of Relative Growth," by Julian S. Huxley, and "The Scientific Basis of Evolution," by Thomas Hunt Morgan, which bring us up to date in the latest British and American thought as to the nature and causes of evolution. They are popularly written and the chief impression they convey is their reversion to more or less pure Darwinism, especially surprising on the part of one of the authors, T. H. Morgan, who some years ago wrote a severe critique of Darwin's theory of adaptation.

We thus have presentations by a distinguished physiological chemist, by the leader of the experimental and genetic school, by an experienced zoologist, P. Chalmers Mitchell, and by one of the leading British authorities on animal life, Julian Huxley. The point of view shown in Julian Huxley's volume should be supplemented by his article in the recent edition of the "Encyclopaedia Britannica."

Preceding a critique of these volumes may we point out four historic explanations of the modes and causes of evolution.

EMPEDOCLES–DARWIN HYPOTHESIS

This bit of absolutely inductive research has a 2,500 year speculative background because about 600 B. C., as described in my volume, "From the Greeks to Darwin," the Greeks began to speculate not only on the modes or kinds of evolution but on the hypothetical causes of evolution. Thus Empedocles of Agrigentum, a Sicilian town, anticipated what may be known as pure Darwinism, namely, that out of many kinds of accidents and variations more or less spontaneously occurring in animals, nature permits the survival only

[1] Address before the Osborn Research Club in the American Museum of Natural History, December 13, 1932.

of those which happen to fit in with the environmental times. This original very crude idea has come down through the ages of human thought, being modernized step by step until it finds its present refinement and up-to-date re-definition and re-statement in the four volumes before us. After many vicissitudes Darwinism is now once more flourishing.

Erasmus Darwin–Lamarck Hypothesis

Similarly, the second great hypothesis as to the modes and causes of evolution, now known as Lamarckism, was adumbrated in Greek time because, as pointed out by Brooks in his "The Foundations of Zoology," the notion that bodily improvement, through the universally acknowledged individual adaptation which comes about through the skilful use of parts, as well as the counteraction of bodily degeneration through disuse of parts, has been discussed down through the centuries and reached its apogee in the mind of Erasmus Darwin, of Lamarck, its modernized ideas in the speculations of Herbert Spencer, and its mechanical climax in the writings of our own Cope. Relative to the ever-growing and verdant Darwinism, Lamarckism is decidedly moribund. It received its death thrust in 1880 when Weismann challenged every kind of evidence for the inheritance of acquired characters. It seems cowardly to attack a dying principle, but I may claim in self-defense that the Titanothere Monograph as well as the more recent Proboscidea Memoir give Lamarckism its final *coup de grace.*

Buffon–St. Hilaire Hypothesis

The third great historic explanation of the nature and causes of evolution appears to have dawned upon the human mind at a relatively recent period, namely, that of the direct action of a favorable or unfavorable, or even of a new environment, on the body and indirectly on the germ as well, although it must be recalled that in all early speculations the sharp distinction which Weismann was the first to draw between the body cells and the germ cells was not clear. Nevertheless, the dictum of Buffon that the mammals of the New World were *dénaturés* as compared with their relatives in the Old World, followed by the violent physicochemical transformation involved in the speculations of Geoffroy St. Hilaire, fully foreshadows the modern speculations, experiments and observations not only on the final inheritance by the germ of the modifying influences of climate but also on the reaction of the germ and consequent origin of hereditary mutations under more or less violent physicochemical agencies.

Aristotle–Driesch Hypothesis

The fourth great principle is purely speculative. It is not truly an induction from observed facts like the others. It is rather a deduction. Hydra-headed, it appears under new designations from the "entelechy" of Aristotle (384–322 B. C.) to the modern "holism" of Smuts or the "emergence" of C. Lloyd Morgan. Perhaps the most expressive designation is that of "vitalism," namely, that there is in life a mysterious self-perfecting principle, charmingly designated by Bergson the "élan vital." Needless to say, this is what the Latins called a *petitio principi;* it is begging the question and avoiding the difficulties by assuming the presence of this internal perfecting tendency. It is something like the assumption by older chemists of "phlogiston" as an explanation of the internal heat and of motion of the body.

Again, while stoutly maintaining that the entelechy of Aristotle and his successors is a theoretic assumption, we should by no means shut out the possibility of further observational or inductive demonstration that there is in life something in the nature of an internal perfecting principle. It is certainly rash at the present moment to deny the possibility of such future discovery. Certainly if there is an *élan vital* it should appear in the creative origin of new parts and organs, as, for example, in the beautiful eyes of the scallop *Pecten* which, as Bergson showed, present so many analogies to the human eye in the position of the lens and retina.

Trends of Recent Thought

Unlike pure Darwinism, which is still greatly debated as an adequate explanation of evolution, and unlike Lamarckism, which is moribund except in the minds of a very few of its living advocates, the Buffon-St. Hilaire principle of direct environmental action both on body and germ is now universally admitted as one of the great causes of evolution. As shown in the experiments of Sumner, one of my former students, it is directly responsible for speciation in animals like *Peromyscus.* Sumner has positively demonstrated that modifications in color and form and proportion, traceable to the prolonged direct action of environment, are hereditary and therefore true germinal characters. Perhaps the best established zoological generalization of modern times is that subspeciation, and ultimately full speciation, is the inevitable result of prolonged change of environment especially visible in color, in proportion, and inevitable in habit which is the invariable precursor of change of form.

The three above principles—Darwinian, Lamarckian and Buffonian—rest upon both a purely speculative and a largely observational basis, and great zoologists, like Edward B. Poulton, who have devoted their entire lives to observations tending to establish pure Darwinism, firmly believe that the pure Darwin principle explains not only color evolution as seen in

protective and aggressive mimicry, but that *all evolution of every kind is explainable by Darwinism.* This is very evident in the years of early correspondence which I enjoyed with Poulton, and our opposing views ripened into full presentation at the centenary of the British Association held in the British Museum a year ago.

My own position with respect to these three historic explanations has been frequently and clearly stated, but I may now briefly summarize it, namely, that pure Darwinism never sought to explain the origin of new characters. In fact, Darwin invariably used the word "chance" but open-mindedly declared that "chance" was a term which might simply express the ignorance of his time as to principles of origin which might subsequently be discovered. Since Darwinism is the only explanation of certain kinds of adaptations and since it is universally admitted that the survival of the fittest is a universal principle and that this applies to fitness in every organ of the body, we all gladly embrace pure Darwinism as one of the great factors of evolution.

As to Lamarckism, the case is quite different. The immediate inheritance by the next generation of the effects of individual adaptation is absolutely disproved, but in the age long or secular biomechanical adaptations of animals we do perceive a great principle which comes within the generic conception of Lamarckism, if not within the specific conception of Lamarckism which the great French naturalist advocated.

As regards the third great principle, namely, the direct action of environment, it seems to be no longer debatable. It is equally evident in every type of animal and plant which we may observe, but, like Darwinism, so far as we know at present it is confined to the modification of existing organs rather than to the origin of new organs, and even as regards the modification of existing organs, the direct action of environment is rather limited.

As a youthful observer I was first strongly impressed by Lamarckism and my early writings led to my classification with Cope as a neo-Lamarckian, but whereas Cope stuck to this explanation of the origin of all biomechanical adaptations to the very end of his life I soon abandoned it and took the speculative ground that there was an entirely unknown factor of evolution awaiting discovery. Fifty years of continuous and very close observation, often of the most laborious and tedious kind, aided by such a splendid assistant as William King Gregory, have only confirmed and strengthened my youthful conviction that the real underlying causes of evolution are entirely unknown, and my present feeling is that they are not only unknown but may prove to be unknowable. The universally and marvelously adaptive principle in the origin and development of biomechanical fitness may be something of the same nature as Newton's principle of gravitation. We may observe all its modes, workings and laws and be able to formulate them in detail even to the great test of prediction. But we may never know the underlying nature of the thing itself. As in gravitation so it may be in biology, some Einstein may succeed a Newton, and yet leave gravitation still in its simpler expression like,—the force that holds the moon in a constantly stable relation to the earth,—something unknown and unknowable, a fact which we can observe but never explain. That is my own present feeling about the whole evolution process.

Having dropped speculation very early in my biologic and paleontologic career, I settled down to the only absolutely safe course for a naturalist, namely, to continual and unremitting observation—day after day, night after night, month after month, year after year. I stick to observation as a shoemaker sticks to his last, through thick and thin, and all the generalizations which I have been able to make are in part confirming generalizations previously made by others, in part discovering entirely new principles hitherto unsuspected. I estimate that 90 per cent. of my time has been given to my own observation and less than 10 per cent. to reading the observations of others. In fact, I am somewhat ashamed of having done comparatively little reading. When I find some one has anticipated me I am only too glad to give him the fullest possible credit, but anticipations are rare because no one in the world has ever had the opportunity afforded me through the splendid financial resources of this museum and the dauntless corps of explorers, field collectors and coworkers who have built up the unrivaled collections in the department of vertebrate paleontology in the American Museum. Such actual documentary records of evolution as have never been accessible before to any naturalist or any observer, however keen, have opened the way to all which may be claimed as new discovery.

Recently Julian Huxley visited the Hall of the Age of Mammals and I showed him the small case two feet square which contains the entire fossil vertebrate collection of the Tertiary, of the year 1891. Then when I told him of the six great exhibition halls of the Life History of the Earth which will be overcrowded when all our existing collections are displayed in them, he was simply amazed. It is not the magnitude of these collections, but the exhaustive and refined way in which they have been assembled with extremely precise field records both as to locality and as to geologic level, which is of inestimable value. This minute observation of geologic sequence, first

in America and then in the Tertiaries of India by Barnum Brown, has brought us to the point where we can demonstrate beyond refutation the absolute origins and continuous developments of new characters. Such observations were undreamt of by Buffon, by Lamarck or by Darwin.

THE VALUE OF THE DETERMINATION OF FREE ENERGY CHANGE FOR ORGANIC REACTIONS[1]

By Professor D. B. KEYES

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IT is only within recent years that the importance of free energy determinations of organic compounds has been realized.[2]

The study of any chemical reaction resolves itself primarily into two fundamental questions: first, how nearly to completion does it go under various conditions, and second, how rapidly does it approach this limit in the presence of various catalysts. The answers to these two quesitons are to be found by means of free energy values.

The well-known formula $\Delta F.^\circ = -RT \ln K$ gives the relation between the equilibrium constant (K) and the change of free energy for the particular reaction under certain definite conditions. The value $\Delta F.^\circ$ is of more general significance than the value K and that is why this particular physical constant is usually sought. The factor R in the equation is the gas constant and T the absolute temperature

It is not necessary to determine the equilibrium constant directly under certain definite conditions in order to find $\Delta F.^\circ$ It is usually quite feasible to perform operations with various chemical equations in free energy terms, so that a numerical value of $\Delta F.^\circ$ is obtained.

It is sometimes quite expedient to use another well-known formula for the determination of $\Delta F.^\circ$ This is: $\Delta F^\circ = \Delta H - T \Delta S$.[3] In this case use is not made of the gas constant R and the equilibrium constant K, but the heat term ΔH and a change in entropy ΔS. The term ΔH is a quantitative value of the heat given off or taken up during the reaction. This heat term is usually measured while the system is at constant pressure. The reaction, however, may be from one physical state to another and not necessarily chemical.

The change in entropy ΔS is made up of a series of ΔS's from the absolute zero up to the temperature existing at equilibrium for each constituent. For example, a solid in heating up from absolute zero to some higher temperature acquires an entropy value equal to the integral of the function $\frac{Cp}{T}$ over the temperature range, where Cp is the specific heat at constant pressure. When the solid changes from the solid state to liquid state the heat of fusion divided by the absolute temperature of melting will constitute the entropy change for that change in state. From then on the specific heat of the liquid is the determining factor until the boiling point of the liquid is reached and then the latent heat of evaporation must be taken into consideration. In this way it is possible to determine from specific heats and latent heat data the S for any particular chemical compound at any definite state and condition. The ΔS for a reaction is calculated from the S values of individual compounds in exactly the same manner as ΔF. In other words, the ΔS value for the reaction is calculated by subtracting the sum of the S values of the reactants from the sum of the S values of the products.

It should be possible theoretically to measure the equilibrium constant of any chemical reaction and from this to obtain easily a $\Delta F.^\circ$ value. We would then have an answer to the question, what are the equilibrium conditions of the reaction, and thereby be able to determine whether or not the reaction was suitable for any particular purpose. Unfortunately, there are a great many practical difficulties that arise, especially with organic reactions.

Reactions studied by the equilibrium method have been those that, first, gave a measurable concentration of product and left a measurable concentration of reactant; second, involved relatively simple methods of analysis; third, had no complicating side reactions. With the exception of ionic reactions, as great a proportion of inorganic as organic reactions satisfy the first requirement—although that proportion is very small indeed. Analytical methods are much better developed for inorganic than for organic compounds. As for side reactions, the distinctive ability of carbon to combine with itself tends to cause much greater

[1] Read before the National Academy of Sciences, University of Michigan, on November 14, 1932.

[2] See American Chemical Society Monograph No. 60, entitled "Free Energies of Some Organic Compounds," by Parks and Huffman, recently published by the Chemical Catalog Company in New York.

[3] $\Delta F = \Delta H - T \Delta S$ under any conditions. $\Delta F = \Delta F^\circ$ when all reactants and products are under one atmosphere pressure.

difficulty with organic compounds. The reactions studied so far, then, have been largely inorganic.

It is well known that the equilibrium constant is determined by the ratio of the activities, and the activities in turn bear definite relations to concentrations of the substances present.

It can, therefore, easily be seen why it is difficult to obtain an accurate determination of the equilibrium constant directly whenever the equilibrium is such that some of these activities and the corresponding concentrations become extremely small. Such a condition quite often occurs when organic chemical reactions are considered.

Organic chemical reactions, on the other hand, are attracting considerably more interest in the industrial world to-day than do the inorganic chemical reactions, because our industries are developing much more along organic lines than along inorganic lines. This means, however, that the determination of equilibrium constants or better yet, free energy values, is becoming more and more important from the standpoint of the applied scientist.

It is interesting to note that in any of this work it is not only desirable but sometimes essential to investigate the catalytic situation involving the particular reaction and determine roughly the rate of reaction under certain definite conditions. This means that the determination of a free energy value for a specific organic reaction usually results in not only determining how far the reaction will go under certain definite conditions, but also how fast. In other words, studies of this kind usually give a fairly complete answer to the fundamental questions concerning the chemical reaction.

We may generally assume in most organic chemical reactions that it is impractical to measure the equilibrium constant directly and thereby determine the free energy for the reaction under certain definite and practical conditions. It is usually necessary to determine the Δ F.° values for a combination of reactions which, added and subtracted from one another will result in the Δ F.° value required as has been shown before.

Free energy change can sometimes be determined, especially in inorganic reactions, by setting up a reversible cell. The equation then becomes $\Delta F.^\circ = -nFE^\circ$. In this equation, n is the number of equivalents, the F is a Faraday and the E° is the potential under standard conditions. In order to set up such a cell it is necessary that the electrons be mobile; that the atoms be capable of giving off and taking up electrons readily. This condition, especially of the carbon atom, is not common and therefore organic reactions do not readily lend themselves to this method of study.

The most practical methods of determining the Δ F.° for organic reactions is the use of the entropy equation previously described. The reason for this is, the specific heats, latent heats, temperatures and heat change during the reaction can be measured usually with more accuracy than other physical constants.

Some preliminary results obtained by actual work of this character on certain interesting organic reactions in the industrial division of the chemistry department, University of Illinois, will be given below.

An attempt has been made to determine the Δ F.° of the reaction between ethylene and water to produce ethyl alcohol. This reaction is particularly valuable because ethyl alcohol is the most important organic solvent at the present time. It is largely made by the fermentation of molasses, which involves rather large equipment cost. A study has been made of various synthetic methods during the last fourteen years, and it is believed that the action of water on ethylene has a most promising future.

The decomposition of ethyl alcohol to produce ethylene and water was first studied largely to determine what would be a satisfactory catalyst. The best catalyst found was aluminum oxide (Al_2O_3), impregnated with about 3 per cent. of phosphoric acid (H_3PO_4). Preliminary results indicated a decomposition of approximately 96 per cent. at 420–430° C. (700° K.), at a space velocity of 100. A small amount of aldehyde, ether and other by-products was found but not determined quantitatively. The $\Delta F.^\circ_{700K}$, based on this 96 per cent decomposition, represented a value of +3,500 cal.

Parks and Huffman[4] indicate a value of +17,900 for $\Delta F.^\circ_{700K}$. Francis and Kleinschmidt[5] give a value for the same ΔF.° of +7,650. Frost[6] indicates still another value for this physical constant of +6,008.

Our figure of 3,500 is undoubtedly low because the amount of undecomposed ethyl alcohol was probably less than the 4 per cent. indicated by the decomposition figures. There is no question, however, regarding the sign, and the probabilities are that the order of magnitude is also correct.

The difficulty with the direct determination proved to be the inaccuracy of methods of analysis pertaining to the products of the reaction.

Considerable time was spent in an attempt to improve these analyses, with unsatisfactory results. The method of attack on this problem at the present time is to divide the reaction into two steps: (1) The absorption of ethylene in concentrated sulfuric acid,

[4] A. C. S. Monograph, "Free Energies of Some Organic Compounds," 123, 1932.

[5] "Applications of Thermodynamics," *Oil and Gas Jour.*, 25: 118, 1929.

[6] "Hydration of Olefins to Alcohols," *Zhur. Pridkladnoikhim*, 3: 1069–76, 1930.

and (2) the production of ethyl alcohol formed from the ethyl acid sulfate by further dilution. The important physical data to obtain in this case are the partial pressures of the reacting constituents.

Another reaction that is of real interest both to the scientific man and the applied scientific man is the reaction between propylene and hydrogen sulfide to produce an alkyl acid sulfide. A direct determination of the equilibrium constant at 573° K. indicated a Δ F.° of +3,000 cal. There are no data in the literature that would indicate the accuracy of this determination.

Considerable polymerization of the propylene occurred in the presence of catalysts such as phosphoric acid on activated charcoal. A quantitative analysis of this polymer is extremely difficult.

The study at the present time is along two lines: (1) the development of a catalyst that will eliminate this and other side reactions, and (2) the determination of the Δ F.° by means of heat capacity measurements. Both methods hold out some promise.

Still another reaction which is of interest to many is the reaction between ammonia and ethylene to produce ethylamine. A study was first made of the reaction itself in order to determine directly the equilibrium constant. A silent electric discharge was used in the place of a catalyst. At 298° K., Δ F.° found from these data was approximately +1,200 cal.

Again the difficulty was due to analytical methods, especially those involving the determination of the amount of ethylamine formed. There were indications of side reactions taking place. Unknown amines apparently were formed. Preliminary results do, however, agree with some rather qualitative work done on the reaction by Francesconi and Ciurlo.[7] It will, of course, be necessary to determine by what, if any, amount the equilibrium is displaced by the discharge.

Further work will be done in the hopes of improving the methods of analysis. If these fail, an attempt will be made to procure a catalyst that will eliminate the side reactions or the Δ F.° will be determined by heat capacity measurements.

The author wishes to acknowledge the assistance of Messrs. M. Gallagher, H. R. Duffey and F. T. Barr, who have done the experimental work and who have contributed many worth-while ideas.

OBITUARY

WILLIAM JACOB HOLLAND

WITH the death of Dr. William J. Holland, which occurred on December 13, 1932, a distinguished figure in the world of science passed away. He was the dean of American entomologists, the author of innumerable publications in this field. But Dr. Holland was far more than an eminent entomologist of world-wide reputation—he was a naturalist of a universality of erudition which is but rarely found among scientific men of the present day. With a prodigious memory, a keen understanding of the diversity of scientific problems, he was at home in the manifold domains of learning. Above all, he was a man of outstanding intellectual and spiritual culture, and that is why his loss has created an irreparable void in the community with which he has been associated for nearly three scores of years and within the ranks of his fellow workers in the scientific field.

William Jacob Holland was born in Bethany, on the island of Jamaica, on August 16, 1848. His family was of Moravian extraction, residing for a long time in Salem, North Carolina. From there his father was sent as a missionary to the West Indies.

From early boyhood Holland was trained in studies of natural history. Upon graduation from the Moravian College and Theological Seminary at Bethlehem, Pennsylvania, in 1867, he received the degree of bachelor of arts from Amherst College in 1869. For a year after graduation he served as principal of a high school in Amherst, and the following year occupied the same position at Westboro, Massachusetts. Having been ordained into the Moravian ministry he entered Princeton Theological Seminary and concluded the course in 1874. Subsequently he joined the Presbytery of Monmouth and came to Pittsburgh as pastor of the Bellefield Presbyterian Church, which position he held until 1891. In the course of his pastorate Dr. Holland devoted much time to scientific studies. He went to Japan as a member of the United States Eclipse Expedition in 1887 and used this opportunity in a very profitable way for various biological investigations. In 1891 he was made chancellor of the Western University of Pennsylvania, now the University of Pittsburgh. In 1898 his friend, Andrew Carnegie, invited him to assume the responsibilities as director of the museum founded by this steel magnate. This office was held by Dr. Holland with signal success until 1922, when he became director emeritus of the Carnegie Museum. Under the administration of Dr. Holland the museum attained the rank of one of the most important scientific institutions of its kind on the continent. Dr. Holland performed the duties as vice-president of the Carnegie Hero Fund from 1904 to 1922, and upon his election as president of that body became a member of the Carnegie Corporation.

[7] Francesconi and Ciurlo, *Gazz. chim. ital.*, 53: 598, 1923.

Dr. Holland was an active member of the board of trustees of several institutions of higher learning, and carried on the duties of Belgian Consul for some years after the world war. He had the distinction of becoming the founder of the American Association of Museums in 1907, and remained president of the association until 1909. He was a member of scores of scientific societies, among them the Zoological and Entomological Societies of London, the Entomological Societies of America, Washington, New York, Cambridge, Germany, France, Russia and Brazil; the American Zoological Society, Royal Society of Edinburgh, American Philosophical Society and several foreign scientific academies. He was a councilor for the Association for International Conciliation, a member of the Academy of Natural Sciences, Philadelphia, the Pennsylvania Historical Society, Historical Society of Western Pennsylvania and Moravian Historical Society.

Dr. Holland was the author of numerous scientific treatises and books, notably of "The Butterfly Book" and "The Moth Book," which became sources of reference for specialists and were chiefly instrumental in stimulating a wide-spread interest in lepidopterology among amateurs in this country. He also wrote many scientific papers published by the United States Government and the Zoological Society of London. The Encyclopedia Britannica applied to him as to a leading specialist in the museum field for the preparation of a survey of the history of scientific museums. For the last thirty-four years he edited the *Annals* and *Memoirs* of the Carnegie Museum. Washington and Jefferson College, Amherst College, Dickinson College, New York University, Bethany College, St. Andrew's in Scotland and the University of Pittsburgh conferred degrees of honorary doctorate upon him.

One of the most significant scientific achievements of Dr. Holland was connected with the paleontological explorations of the Carnegie Museum which were directed by him in Utah, Wyoming, Montana and North Dakota. The finds of these expeditions resulted in many discoveries, including the celebrated *Diplodocus carnegiei*, the original skeleton of which is adorning the gallery of fossils in the Carnegie Museum, whereas nine replicas were presented to the leading scientific institutions in Europe and both Americas. In recognition of his contributions to science Dr. Holland received decorations from Belgium, Austria-Hungary, Italy, Russia and Spain.

In his busy life, Dr. Holland found time to attend to many public responsibilities of diversified nature. For instance, he took an active part in the development of the water system in the City of Pittsburgh.

Dr. Holland passed away amid his favorite labors, lending his concentrated attention, as throughout his life, to various problems of natural history advanced by recent investigations. Until the end he remained absorbed by the adventurous spirit of scientific research, responsive to the lure of exploring some new avenue of the ever-widening horizons of knowledge. He lived a life of exceptional usefulness. Endowed with a profusion of gifts, among which his linguistic abilities were by far not the least, he cultivated his native talents and molded them into a personality of unique qualities and values. A churchman, a leader in education, the father of the Carnegie Museum in Pittsburgh, a man of learning honored far and wide by academies, international congresses and universities, Dr. Holland will not only be mourned sincerely, but his achievements will also not fail to remain an inspiration challenging the best within us.

A. AVINOFF

CARNEGIE MUSEUM
PITTSBURGH, PENNSYLVANIA

RECENT DEATHS

GEORGE P. ADAMSON, chemist and formerly director of research of the General Chemical Company, New York City, died on February 16. He was sixty-eight years old.

DR. CLARENCE HENRY ECKLES, chief of the division of dairy husbandry at the University of Minnesota, died on February 13, at the age of fifty-eight years.

DR. CARL L. CORRENS, honorary professor of botany at the University of Berlin and formerly director of the Kaiser Wilhelm Institute for Biology, has died at the age of sixty-eight years.

HORATIO WARD STEBBENS, associate professor of mechanical engineering at Stanford University, died at the Stanford Hospital in San Francisco on February 2, following an operation several days before. Professor Stebbens, who has been at Stanford since 1914, was graduated from the University of California and the Massachusetts Institute of Technology, and prior to teaching, had been connected with various engineering projects.

THEODORE SAUNDERS DUNN, associate professor of mining engineering at Stanford University, was killed instantly when struck by an automobile on the Stanford campus the evening of February 3. Professor Dunn was a graduate of the Missouri School of Mines, and besides having practical experience with a number of mining corporations, and serving as a captain in the engineers during the World War, had served on the faculties of the Missouri School of Mines, South Dakota School of Mines and Georgia School of Technology, going to Stanford in 1930.

SCIENTIFIC EVENTS

DANISH EXHIBIT AT THE CHICAGO MUSEUM OF SCIENCE AND INDUSTRY

WORD has been received from Dr. Max Henius, of Chicago, that the Ways and Means Committee of the Danish government has approved the expenditure of fifty thousand kroner for exhibits for the Museum of Science and Industry in Chicago. These exhibits are to portray the contributions of Danish scientists to world progress. Dr. Henius, who has been director of the Wahl-Henius Institute in Chicago for many years, has already secured a large operating model of a brewery for the museum. He has also been very active in the establishment of the Dan-American Archives, which are a gift of the American people to the people of Denmark.

The exhibits from Denmark will consist of originals and replicas of the apparatus used by Denmark's great scientists and engineers from Tycho Brahe in the sixteenth century to Knudsen, Poulsen and Bohr—Denmark's leaders in modern science. Tycho Brahe was one of the first great observers of the motions of the sun, moon, stars and planets. The observatories which he built and the instruments which he devised form one of the most precious heritages of modern astronomy. A model of his great observatory at Uraniborg as well as models of his instruments will form part of this exhibit.

It is also fitting that Chicago, the home for so long of Dr. Albert A. Michelson, whose measurements of light won him the Nobel prize, should receive from Denmark examples of the work of Ole Römer. Römer invented the transit circle and by his measurements of the light from Jupiter he was able to pronounce for the first time that light traveled at a definite speed and not at an infinite speed as had been supposed. Römer in 1676 estimated the speed of light at 192,000 miles a second and Michelson in 1926 corrected this to approximately 186,000.

In the field of electricity Hans Christian Oersted ranks with Michael Faraday. His discovery of electromagnetism forms, together with Faraday's discovery of electromagnetic induction, the foundation of the age of electric power. During his great career Oersted founded both the Danish Society for the Propagation of Science and the Polytechnic Institute. Replicas of his original apparatus, of his electromagnet, and a copy of his essay on his great discovery will form part of the Danish gift to the Museum of Science and Industry. A bronze tablet commemorating the centenary of Oersted's discovery of electromagnetism has already been given to the museum by Dr. Neils C. Oatved, of Detroit, Michigan.

Among these exhibits will also be included an early model and a modern telegraphone, the invention of the Danish scientist Waldemar Poulsen. The telegraphone, which has recently come into use again in Europe for recording telephone messages when the party called is absent, is a device which records sound on an iron wire. Poulsen's principal contribution to scientific progress which will be shown among this group of exhibits was his production in 1903 of an oscillator which made continuous radio waves possible.

No exhibition of the work of Danish scientists would be complete without that of Niels Bohr. Although the atomic theory has changed since Bohr received the Nobel Prize for his work, the rise of atomic physics dates back to him and every new discovery in the field serves to emphasize the task that Bohr performed in directing the thinking of scientists all over the world along these lines. Hence the Museum of Science and Industry has asked that the Danish government include the work of Bohr in this gift of exhibits.

The exhibits from Denmark will be housed temporarily in the Hall of Science of A Century of Progress. Although the Museum of Science and Industry will be opened to the public in May, 1933, the decision has been made to loan the Danish exhibits to A Century of Progress for six months before placing them permanently in the museum.

CORRESPONDENT

THE HERBARIUM OF W. W. ASHE

THE University of North Carolina has secured the W. W. Ashe Herbarium, a collection very valuable for its large number of type specimens from the southeastern states. William Willard Ashe (1872–1932), a graduate of the University of North Carolina and Cornell University, was the first forester employed by the state of North Carolina. After eighteen years in forestry service for North Carolina he spent twenty-three years in the U. S. Forest Service. He was a keen observer and the qualities that carried him far in his vocation of forestry also made him an important systematic botanist. He is credited with 179 scientific papers, about one third of them in systematic botany. In these he published 510 new botanical names (including species, varieties and forms).

The acquisition of the Ashe Herbarium by the University of North Carolina was made possible through the generosity of Mr. George Watts Hill, of Durham, N. C. Most of it is unmounted; but Mr. Hill's gift made possible not only the purchase of the plants, but the cost of mounting them and the purchase of suitable herbarium cases. To assist in assembling and mounting the herbarium, the university has secured

the services of Mr. T. G. Harbison, of Highlands, N. C. He was for years a collector for the Biltmore Herbarium, a collector for Sargent for over a quarter of a century, is the author of a number of papers on systematic botany, and as a friend and fellow collector of Mr. Ashe he is familiar with Mr. Ashe's signs and symbols. With Mr. Harbison's assistance the herbarium should be ready for the use of visiting botanists by summer.

Through the generosity of Mrs. W. W. Ashe the university is also the recipient of a large number of Mr. Ashe's reprints of botanical and forestry interest and a number of botanical journals and books.

H. R. TOTTEN

HECKSCHER GRANTS AT CORNELL UNIVERSITY

THE Heckscher Research Council of Cornell University has announced that supplementary grants amounting to about $11,000 have been made for the continuance of eighteen scientific researches.

The allocation of grants followed a gift of $10,000 from the Carnegie Corporation made last autumn. A special gift of $1,000 made by Mrs. Harry Snyder, of Minneapolis, for the promotion of research in biochemistry and allied fields at Cornell was also allocated at this time to the continuance of researches assisted by an earlier gift from Mrs. Snyder.

The Heckscher Foundation for the promotion of research at Cornell was established by Mr. August Heckscher, a trustee of the university, in 1920 by a gift of $500,000. During the twelve years it has been in existence the fund has provided an income of $445,000 for the support of 200 separate projects.

The following supplementary grants were made for the academic year, ending June 30, 1933:

Professor Wilder D. Bancroft: For researches in photochemistry.

Professor Samuel L. Boothroyd, '04–5, G: To continue work on meteors.

Professor T. Roland Briggs, '09, and Carleton C. Murdock, '12: For a study of the size and shape of colloidal particles with special reference to catalytic agents.

Professor L. M. Dennis: For investigation of rare elements.

Professor R. Clifton Gibbs, '07: For a study of line spectra in the extreme ultra-violet.

Professor Edwin F. Hopkins, '15: For a study of the physiological effect of iron and certain other elements in the ionized state.

Professor Earle H. Kennard, '13, Ph.D.: For research in theoretical physics.

Professor Robert Matheson, 06–7: For a study of culicid ecology.

George Maughan: To continue study of the effects of ultra-violet light on animal physiology.

Professor Leonard A. Maynard, '15, Ph.D., and C. M. McCay: To continue investigations on physiological effects of purified diets in herbivora, and to continue investigations on biochemical changes that accompany aging in the animal body.

Professor Ernest Merritt, '86: For study of the influence of the conditions of the upper atmosphere on the transmission of electric waves.

Professor Murdock: For work in x-rays.

Professors Edward L. Nichols and Ernest Merritt: For studies in luminescence.

Professor Jacob Papish, '21, Ph.D.: For a study of the occurrence, distribution and association of the rarer chemical elements.

Professor Otto Rahn: For studies of radiation from living matter.

Professor Hugh D. Reed, '99, Alan C. Fraser, '13, and George C. Embody, '10: For the purpose of undertaking genetical studies and related problems in fishes.

Professor Floyd K. Richtmyer, '04: For investigations in the laws of absorption of x-rays.

THE AMERICAN JOURNAL OF SCIENCE

DR. RICHARD S. LULL, Sterling professor of paleontology and director of the Peabody Museum of Natural History at Yale University, has been appointed editor of *The American Journal of Science,* succeeding Dr. Ernest Howe, who died in December.

The American Journal of Science and Arts was established by Benjamin Silliman in 1818, and is an integral part of the educational activities of Yale University. This was the first serial scientific periodical in this country as Silliman was the first professor of science. At that time the scientist had only the irregular publications of a few scientific societies or academies in which to bring out with some promptness the results of his work.

As planned by Silliman *The American Journal* was to embrace all branches of science, "more especially mineralogy and geology"; including also "the ornamental as well as the useful arts." Papers on these last subjects appeared occasionally in early volumes, but more and more infrequently and finally, in 1880, "the Arts" was dropped from the title. The wide range of subjects was gradually somewhat restricted and after the 1890's articles outside of the geological field were more and more infrequent. The other subjects (as chemistry, physics, botany, etc.) still find a place in the abstracts of papers published elsewhere, or in the reviews of books in all lines of science.

The elder Silliman carried all the work of the *Journal* for some twenty years till his son, Benjamin Silliman, Jr., came in to assist him in 1838. A little later (1846), his son-in-law, James Dwight Dana, was also included in the editorial board. The last soon took upon himself the entire work and carried this on until within a few years of his death in 1895. For years previous to this he had had the assistance of his

son, Edward S. Dana (whose name was included among the editors as early as 1875). From 1895 until 1925, the name of the younger Dana alone appeared as editor-in-chief. In 1925–26, Professor Alan M. Bateman rendered essential assistance until, through his efforts, Dr. Ernest Howe became editor in 1926.

The associate editors of *The American Journal* have been of importance in their support in many directions, especially by their contributions of abstracts of original papers published elsewhere. The list of associate editors from 1851 to the present time includes the names of many of the most distinguished scientists of the time, chiefly in this country.

At the time of the hundredth anniversary of the *Journal* in 1918 a number of lectures, certain of which were made the basis of a series of Silliman Lectures at Yale, were gathered together and published under the title, "A Century of Science in America, with special reference to *The American Journal of Science*, 1818–1918."

The ownership of the *Journal* passed to Yale University in 1926 through the gift from the editor and owner, Edward S. Dana. An endowment has been gradually accumulated, a large part of which has been given by the immediate descendants of the founder of the *Journal*.

SCIENTIFIC NOTES AND NEWS

SIR CHARLES SHERRINGTON, Waynflete professor of physiology at the University of Oxford, has been elected an associate member of the French Academy of Medicine.

DR. H. SPENCER JONES, recently appointed Astronomer Royal at the Greenwich Observatory, formerly a fellow of Jesus College, Cambridge, has been elected to an honorary fellowship.

DR. GILBERT THOMAS MORGAN, director of the chemical research laboratory of the Department of Scientific and Industrial Research at Teddington, has been nominated to the presidency of the Chemical Society, London.

DR. NEVIL VINCENT SIDGWICK, university reader in chemistry at the University of Oxford, fellow of Lincoln College, Oxford, and member of the Advisory Council of Scientific and Industrial Research, has been elected a member of the Athenaeum Club, under the provision which empowers the annual election of a certain number of persons of distinguished eminence in science, literature, the arts or public service.

AT the annual dinner on February 22 of the American Institute of Mining and Metallurgical Engineers, the Saunders Medal, for distinguished achievement in mining engineering, was presented to Walter H. Aldridge, president of the Texas Gulf Sulphur Company, and the James Douglas Medal, for distinction in metallurgy, was presented to James O. Elton, manager of the International Smelting Company of Salt Lake City.

DR. FRANK C. MANN, of the Mayo Foundation, Rochester, Minnesota, has been awarded the William Wood Gerhard gold medal of the Pathological Society of Philadelphia for 1932. Dr. Mann also gave the annual conversational lecture on the evening of February 9. His subject was "Observation on Experimental Pathology and Pathologic Physiology of the Liver."

OIL paintings of Dr. Charles N. Gould and Dr. Charles E. Decker, commemorating their many years of active service in connection with the development of geological science in Oklahoma, were unveiled on February 8 at the University of Oklahoma. Thirty-three years ago Dr. Gould organized the department of geology at the university and eight years later organized the Oklahoma Geological Survey. For seventeen years Dr. Decker has taught paleontology at the university. He has also served as secretary-treasurer of the American Association of Petroleum Geologists, and as grand president of Sigma Gamma Epsilon. Dr. Irving Perrine, formerly a member of the faculty of geology, presided. Short addresses were made by a number of former students. President Bizzell, of the university, accepted the paintings, which were presented by Gamma Chapter of Sigma Gamma Epsilon.

DR. WALTER H. EVANS, chief of the Division of Insular Stations of the Office of Experiment Stations, retired on February 1, having reached the compulsory retirement age of seventy years. Dr. Evans was presented by his associates with a signed scroll and a watch, as an appreciation of his long and devoted service and as an expression of their high regard for him in their personal and official relationships.

A DINNER in honor of Dr. Francis M. Pottenger, Monrovia, California, retiring president of the American College of Physicians, was given by Southern California members of the college in Los Angeles, on January 12. Dr. David P. Barr, St. Louis, was the guest speaker.

DR. WILLIAM ALLEN PUSEY, emeritus professor of dermatology, University of Illinois College of Medicine, will deliver the Adolph Gehrmann Memorial Lectures at the Research and Educational Hospital of the college on March 1, 2 and 3, on "The History and Epidemiology of Syphilis." A dinner in honor

of Dr. Pusey has been arranged by the university, to be given on March 2.

PROFESSOR GEO. J. MILLER, State Teachers College, Mankato, Minnesota, was chosen president of the National Council of Geography Teachers at its recent meeting in Washington. The next annual meeting will be held at Northwestern University on December 26 and 27.

AT the January meeting of the Pathological Society of Philadelphia, officers elected for the year 1933 were: Dr. V. H. Moon, *president;* Dr. Morton McCutcheon, *vice-president,* and Dr. Herbert L. Ratcliffe, *secretary-treasurer.*

PROFESSOR WARREN WEAVER, chairman of the department of mathematics of the University of Wisconsin, has resigned his post to accept permanent charge of the Natural Science Division of the Rockefeller Foundation, a post which he assumed temporarily during a year's leave of absence. His resignation has been accepted by the regents, and Professor M. H. Ingraham has been named chairman of the department.

DR. GEOFFREY DOUGLAS HALE CARPENTER has been elected Hope professor of zoology at the University of Oxford, vacant through the retirement of Professor E. B. Poulton, who had held the chair for forty years.

PROFESSOR K. BERINGER, of Heidelberg, has succeeded Professor Bostroem in the chair of psychiatry at Munich.

DR. FRITZ EICHHOLTZ, professor of pharmacology at Königsberg, has been called to Heidelberg.

MR. JOHN L. WIRT retired as bursar of the Carnegie Institution of Washington on February 1. Mr. E. A. Varella succeeds him and Mr. E. B. Biesecker has become assistant bursar.

WALTER G. CAMPBELL, director of regulatory work in the U. S. Department of Agriculture, resigned recently to become chief of the Food and Drug Administration, effective on February 1. The position of director of regulatory work has been abolished, thus completing the reorganization of the department's law-enforcement work started in 1923.

THE Committee on Scientific Research of the American Medical Association has awarded grants in aid to Dr. Philip B. Armstrong, of the Cornell University Medical College, to study the action of drugs in relation to the innervation of the heart; to Professor Wm. C. Rose, of the University of Illinois, for a continuation of his studies on the nutritive importance of the amino acids, with particular reference to the isolation of a hitherto unknown dietary essential present in certain proteins, and to Dr. C. C. Speidel, professor of anatomy in the University of Virginia Medical School, in support of his work on myelinated nerve fibers.

DR. DINSMORE ALTER, professor of astronomy at the University of Kansas, spent the first week of February in Tucson, Arizona, where he conferred with Dr. A. E. Douglass, director of the Seward Observatory, regarding a plan for a joint research into the problem of meteorological cycles.

DR. RALPH H. SMITH, entomologist in the University of California Citrus Experiment Station, visited the lower Rio Grande Valley of Texas during January. He conferred on citrus pest control problems and delivered two lectures on oil sprays. Dr. Smith also visited refineries and petroleum testing establishments in the southern and middle-western states.

DR. P. H. ROLFS, formerly dean of the College of Agriculture at the University of Florida, after twelve years' absence, is returning to take up his residence at Gainesville, Florida. In 1920 he was employed by Minas-Gerais to establish and conduct a state agricultural college. This work was completed in 1928. Since that time he has been serving the state as the technical adviser in agriculture.

DR. GEORG MASING, the German metallurgist, delivered the Institute of Metals Lecture at the annual meeting of the American Institute of Mining and Metallurgical Engineers. Professor George B. Waterhouse, of the Massachusetts Institute of Technology, delivered the annual Howe Lecture.

DR. THORNE M. CARPENTER, of the Nutrition Laboratory of the Carnegie Institution of Washington, gave two lectures in February at Columbus, Ohio, before the Ohio State Chapter of the Society of Sigma Xi. On February 9 he spoke on "The Development of Methods for Determining Basal Metabolism of Mankind" and on February 10 on "Problems and Factors in the Determination of the Basal Metabolism of Man."

PROFESSOR PIET VAN DE KAMP, of the University of Virginia, on February 9 spoke on "The Absorption of Light in the Galactic System," before a combined meeting of the Virginia chapters of Sigma Xi and Phi Beta Kappa.

DR. D. K. TRESSLER, chief chemist of the Birdseye Laboratories, Gloucester, Massachusetts, addressed a joint meeting of the Massachusetts State College Sigma Xi and Graduate Clubs on February 16 on "Recent Researches on the Preservation of Foods."

THE Federation of American Societies for Experimental Biology will meet at Cincinnati, Ohio, on April 10, 11 and 12.

THE American Association of Petroleum Geologists will hold its eighteenth annual meeting at Houston, Texas, on March 23, 24, 25.

THE American Public Health Association announces its sixty-second annual meeting, to be held in Indianapolis, Indiana, from October 9 to 12. It was in Indianapolis in 1900 at the twenty-ninth convention of the American Public Health Association that Dr. Walter Reed read a paper entitled "The Etiology of Yellow Fever—A Preliminary Note" indicating that the mosquito serves as the intermediate host for the parasite of yellow fever. At the coming meeting it is planned to honor the only living participant in the Yellow Fever Experiment, Dr. John R. Kissinger, at a special memorial session. The scientific program will discuss aspects of modern public health practice, from the viewpoint of the health officer, the laboratory worker, the epidemiologist, the child hygienist, the industrial hygienist, the nurse, the vital statistician, the health educator, the food and nutrition expert, the sanitary engineer.

THE Ohio Academy of Science will hold its forty-third annual meeting at Ohio University, Athens, on Friday and Saturday, April 14 and 15, under the presidency of Professor R. A. Budington, of Oberlin College. As in former years, the program will consist of one or two general sessions of the academy and sectional meetings, one or more for each of the seven sections. The general sessions will be devoted partly to business and partly to the discussion of scientific topics of general interest, probably by eminent invited speakers. Friday evening will be given over to the annual dinner at which will be given the presidential address. The section of chemistry will meet for the first time with Professor William Lloyd Evans, of the Ohio State University, Columbus, as the first vice-president. This section begins with an enrolment of sixty-eight members. The Central Ohio Physics Club will meet with the academy again this year, as in the past few years. The Ohio Academy of Science is sponsoring a series of weekly radio talks of fifteen minutes each beginning at 7:15 P. M. each Friday over the Ohio State University broadcasting station, WEAO, covering the four months from January to April.

AFTER annuities and legacies have been paid, the residue of the estate of the late Frederick G. Bonfils, editor of *The Denver Post,* is left to the foundation which he created in December, 1927, as "a corporation, not for profit, organized for charitable, benevolent, scientific, medical and public educational purposes." The value of the estate is said to be estimated at over $10,000,000.

DR. J. MIDDLEMASS HUNT, who held the post of honorary dean of the Liverpool School of Tropical Medicine for many years, has made a bequest of £20,000 to the University of Liverpool to provide for the endowment of the chair of tropical diseases of Africa.

THE Chemistry Club of the Georgia State College for Women, Milledgeville, Georgia, cooperating with the friends of Charles H. Herty, is awarding annually a medal for work done in the field of chemistry in the South. The purpose of the medal is twofold: (1) To give public recognition to worthy research workers in the colleges and laboratories of industry, whose real value is often unappreciated by their institutions. (2) To honor Dr. Herty, who has contributed a great deal to the development of the South, and who was born in a house which stood on what is now the campus of the Georgia State College for Women at Milledgeville. The award will be announced by May 1 of each year, and the medal will be presented at the May meeting of the Georgia Section of The American Chemical Society, which is invited to meet at Milledgeville. A modest allowance will be made to cover expenses of the winner to this meeting, where he will deliver an address.

Museum News reports that the Carnegie Corporation has made a grant of a fellowship to the New York Museum of Science and Industry to allow studies to be made, under the supervision of Professor Edward S. Robinson, of Yale University, of problems peculiar to museums of science and industry. One of the most important problems concerns the comparative mental reactions of young persons of various ages when dealing with exhibits operated by hand and when dealing with automatically operating exhibits which can be studied from a distance slightly beyond arm's reach or behind glass. Others are concerned with the question of what proportion of visitors, particularly young ones, pay attention to the descriptive labels placed on the exhibits and also to the social and economic interpretations on the walls above the machine or apparatus; also, the characteristic reactions of people in this museum as compared to people in art or natural history museums.

ACCORDING to *Museum News* the Museums Association of Great Britain has adopted regulations for gaining its museum curator's diploma. These regulations set forth two aspects of the education of the museum official of which the association takes cognizance. Competent knowledge (a) of at least one subject relating to museum collections and (b) of museum administration, methods and technique. Knowledge of a museum subject must be shown by an earned degree, diploma or certificate from one of a number of specified educational institutions. Knowledge of museum technique is determined by examination, and a

panel of examiners is established by the association for this purpose. Attendance is required for one week each at an elementary, an advanced and a specialized course approved by the association. As a final test a candidate must submit a thesis on museum work and a concrete example of curatorial work. Candidates must have three years experience in a museum or art gallery before the diploma is awarded. Fee for registration is 10s 6d and for taking the diploma one guinea.

DISCUSSION

CORTIN AND TRAUMATIC SHOCK

A NOVEL explanation of the "secret" of traumatic or secondary shock has recently been published in SCIENCE by a group of investigators from the Biological Laboratory of Princeton University.[1] The physiologic phenomena associated with death from adrenal cortex insufficiency have been compared with those found in traumatic shock. The analogy is a close one, as the authors have shown in a list of 32 particulars. Fundamentally, both conditions are characterized by a diminution in the volume of circulating blood, with a failure of the blood-diluting mechanism. The suggestion is made that "the signs and symptoms of adrenal insufficiency, and of traumatic or secondary shock, are possibly due to one and the same thing, namely, failure of the blood volume and blood-diluting regulator mechanism, the adrenal cortex." Because the "adrenalectomized animals, in the absence of the cortical hormone, are apparently unable to draw fluid back into the blood stream through the capillary walls" the writers suggest that "the accepted view that the osmotic power of the blood colloids is the chief factor involved in compensation needs further investigation."

There is only one dissimilarity among the 32 particulars which the writers have listed showing the analogy between adrenal insufficiency and traumatic shock. This discrepancy, as they point out, is the fact that in adrenal insufficiency the blood sugar is low, while in traumatic shock it is normal or elevated. They promise a discussion on this point at some future time. It seems possible, however, that this discrepancy may be of more moment than is realized at first glance. That the secretion of the adrenal cortex exerts a profound influence on the storage and utilization of carbohydrates has been realized for some time. The prepotent function of the cortex of the adrenal is considered by Britton and Silvette[2] to be the regulation of carbohydrate metabolism. They have found that the glycogen in the liver and muscles is lower after adrenalectomy than in extreme inanition, severe exhaustion, after strychnine convulsions or after exposure to cold. They state that "moreover, the muscle glycogen and blood glucose in cases of hepatectomy are not depleted more thoroughly than in animals showing more or less severe symptoms of adrenal insufficiency." It is therefore recognized that the glycogen stores and the blood glucose level are reduced in adrenalectomized animals. If the blood glucose level is reduced by insulin, as shown by Drabkin and Edwards in 1924,[3] certain physiologic reactions occur which are very similar to changes which the investigators have found to occur after adrenalectomy. In insulin hypoglycemia there is an actual loss of blood volume and a considerable concentration of the blood. These animals also show a decline in blood pressure and death from insulin "shock." Is it possible that the death from adrenal insufficiency may result primarily from the alterations in the carbohydrate metabolism which secondarily exert some influence on the blood hydration rather than through some hypothetical control over the mechanism of fluid exchange?

Some interesting experiments are reported by the authors to "test the relation between failure of the cortex and traumatic shock" and the results to them are "highly suggestive." Traumatic shock was produced in adrenalectomized dogs. Administration of cortin promptly resuscitated the dying animals. Control experiments were performed which demonstrated that a similar trauma did not produce shock in normal dogs. Such an experiment is very convincing proof that cortin is a specific therapeutic agent for the shock which is associated with the absence of cortico-adrenal hormone. But quite as specific is the glucose which is injected into animals in insulin shock. No one believes that traumatic shock and insulin shock are due to the same mechanism.

Again, other experiments were performed in which "profound surgical shock was induced in normal dogs by double adrenalectomy at one sitting. Immediately following completion of the operation, the blood pressure was normal." At this point the question may be raised as to their criteria of "profound surgical shock" resulting from operations at the close of which the blood pressure was *normal*. If such animals received cortin they recovered and were normal in 2 to 3 days. Dogs similarly "shocked" and not injected with hormone invariably died within 48 hours. The investigators have clearly demonstrated that the cortico-adrenal hormone is a specific therapeutic agent

[1] W. W. Swingle, J. J. Pfiffner, H. M. Vars, P. A. Bott, W. M. Parkins, SCIENCE, 77: 58, January 13, 1933.

[2] S. W. Britton and H. Silvette, *Am. Jour. Physiol.*, 100: 701, 1932.

[3] D. L. Drabkin and D. J. Edwards, *ibid.*, 70: 273, 1924.

for the shock which is associated with absence of the adrenals, but they have not reported experiments in which the effect of cortin has been assayed in traumatic shock. Always when they produced shock the adrenals were absent. Shock was never produced when the adrenals were present. They conclude, "the idea that the adrenal cortical hormone might prove of benefit in the treatment of human traumatic shock is advanced merely as a suggestion. Adequate proof can only come through clinical trial." Since it is known that deficiency of the cortico-adrenal hormone results in a lowering of the glycogen and blood glucose levels and since it is known that if the blood glucose level is reduced by insulin, there is a reduction in blood volume with blood concentration, the suggestion that the function of the adrenal cortex is the regulation of blood volume and blood dilution seems unnecessary. Although the analogy between deficiency of cortico-adrenal hormone and traumatic shock is a close one, no convincing evidence has been presented that the two conditions have a common etiology or that the cortical hormone is of benefit in the treatment of shock.

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THE ASSOCIATION OF TERMITES WITH FUNGI

A SURVEY of the fungi associated with *Kalotermes minor*, a dry-wood termite, *Reticulitermes hesperus*, the common western subterranean termite, and *Zoötermopsis angusticollis*, the large damp-wood termite, has been made.

Fungi were found to be associated with each of the fifteen colonies of *K. minor*, twelve colonies of *R. hesperus* and twelve colonies of *Z. angusticollis* studied. They were isolated (1) from the exterior of the termites, (2) from the gut of the termites, (3) from the fecal pellets of *K. minor* and *Z. angusticollis*, (4) from the "frass" with which *R. hesperus* plugs up abandoned galleries, (5) from the wood of the inner surface of the termite burrows, and (6) from the wood 1 to 2 mm below the surface of the walls of burrows. Henceforth, in speaking of the isolation of a fungus from a termite colony, this designation is used to include not only the termites themselves, but also their fecal pellets, the frass and the wood enclosing their burrows.

Representatives of thirty-three genera of fungi and twenty undetermined fungi were isolated from the colonies of the three species of termites. A somewhat smaller number of fungi was associated with *K. minor* than with *R. hesperus* or *Z. angusticollis*, seventeen genera of fungi and four undetermined fungi being isolated from twelve colonies of *K. minor* as compared to twenty-five genera and eight undetermined forms from twelve colonies of *R. hesperus* and twenty-two genera and three undetermined forms from twelve colonies of *Z. angusticollis*.

The average number of fungi isolated from ten cultures made from each of twelve colonies of each of the species of termites was 4.91 for *K. minor*, 8.75 for *R. hesperus* and 7.25 for *Z. angusticollis*.

A smaller amount of fungous growth was present in the colonies of *K. minor* than in those of *R. hesperus* or *Z. angusticollis*. Furthermore, the wood containing the *K. minor* colonies usually showed little structural injury from fungous attack, while that enclosing the *R. hesperus* and the *Z. angusticollis* burrows usually showed decay. This is undoubtedly due to the fact that the wood containing the colonies of *K. minor* was drier and, therefore, less favorable to the growth of wood-destroying fungi.

Penicillium and *Trichoderma* were the genera of fungi most frequently isolated from the colonies of each of the three species of termites. There was no evidence of any specific relation between any fungus and a given species of termite.

Termites were placed upon pure cultures of a fungus, and were found capable of transporting large numbers of fungous spores and hyphae on their legs and bodies. Subsequent dissection revealed many entire and fragmented spores and a few fragments of hyphae of the fungus in the gut of the termites.

Fungi were more abundant on the inner surface of termite galleries than in the wood below the surface of gallery walls, only four cultures out of fifteen taken from wood 1 to 10 mm from the galleries of a colony of *K. minor* being positive as compared to sixteen positive cultures out of seventeen taken from the surface of gallery walls.

Twelve cultures made from the heartwood of a pole uninfested by termites were all negative. Seventeen out of twenty-seven made from the sapwood were also negative. In a termite-infested pole, of the same kind of wood and having a similar history of use, fourteen cultures taken at intervals throughout the diameter of the pole from wood adjacent to termite galleries were all positive. The same two fungi which were isolated from the wood near the exterior of the first pole were common throughout the diameter of the second pole in and near the termite galleries. Nine other fungi were also present in the termite-infested pole.

It seems evident, then, that termites may introduce fungi which were not previously present in the wood and that they may aid in the spread through the wood of these and of those already present.

The results of this investigation indicate that con-

ditions in the burrows of *K. minor, R. hesperus* and *Z. angusticollis* are favorable to the growth of fungi and that fungi are present in the walls of the burrows and in the fecal pellets. Termites enlarge their burrows; they eat their fecal pellets. Fungi would seem, then, to be a common element in the diet of the termites. Further investigation, however, will be necessary to determine the significance of fungi as food for termites. Fungi may cause chemical changes in wood. Hence, the question whether their presence may render wood more available or more attractive to the termites also becomes a problem of interest.

A more detailed account of this investigation will appear later.

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ANTIPROTHROMBIN AND GLOBULINS

IN a recent note[1] A. Fischer called attention to his work on the effect of the addition of heparin upon precipitation of proteins, especially of blood proteins, in an acetate-acetic acid solution at pH 5.0. With small additions of heparin there is an increased precipitation of casein, blood serum protein, or a precipitation of serum albumin. With increased amounts of heparin, precipitation may be inhibited or the precipitate dissolved. The addition of heparin to dialyzed serum albumin gives a flocculation at pH 5.0. These conclusions are similar to those previously published by Fischer in the *Biochemische Zeitschrift*.[2] Fischer concludes from his experiment that euglobulin is formed from serum albumin and suggests that euglobulin is formed in the liver through the combination of serum albumin with heparin.

We were interested in these findings of Fischer in connection with the blood of new-born animals. The serum or plasma of such animals does not contain euglobulin according to the ordinary methods of detecting it, and in some animals practically no pseudoglobulin I (if one wishes to recognize two pseudoglobulins). If euglobulin is formed by combination of heparin with albumin, it seemed possible that the failure to find euglobulin might in some way be connected with the concentration of heparin in the newborn blood. Furthermore, if euglobulin is formed by a combination of heparin with albumin, such a serum offers an excellent opportunity to test the fact. It is better than a purified serum albumin, because changes in the character of the protein through the removal of euglobulin and the purification of the albumin likely to occur in any purification process would be reduced to a minimum. The only change involved is the clotting of the fibrinogen.

Heparin preparations, both purified and crude, were presented to us by Hynson, Westcott and Dunning. The addition of heparin to new-born serum or to adult serum with an acetate buffer mixture according to the procedure of Fischer confirmed his results with regard to precipitation at pH 5.0, *i.e.*, a turbidity developed with the new-born serum where no precipitate had formed without heparin and a turbidity appeared with adult serum greater than that obtained without heparin. Furthermore, the pH for maximum precipitation was found to be different for the cow and horse. Additional amounts of heparin after the maximum turbidity was reached resulted in a decreased turbidity. However, the addition of a neutral salt or mixture of salts which precipitate euglobulin, such as 1.00 volume-molar sodium sulfate solution or of 1.425 volume-molar solution of potassium phosphate at approximately pH 7[3] to untreated new-born serum and to new-born serum to which heparin had been added failed to give a precipitation. The concentration of heparin used was that which gave the maximum turbidity at pH 5.0. Furthermore, the addition of 1.50 volume-molar sodium sulfate solution or of 2.025 volume-molar potassium phosphate at approximately pH 7, which precipitate both euglobulin and pseudoglobulin, gave equal precipitations with new-born serum in the presence or absence of heparin.

After these observations were completed a communication by Fischer and Schmitz[4] appeared in which they indicate that ammonium sulfate turbidity curves are not affected by heparin. These authors also state that the addition of herparin to serum and plasma from the same animal causes a difference in the distribution of albumin and globulin of plasma such that the albumin fraction of the plasma is decreased, while the globulin fraction is increased as compared to the corresponding fraction from serum. We have fractioned the new-born serum and plasma from the same animal, with and without the addition of heparin, at 0.75 molar and at 1.50 molar sodium sulfate. These concentrations are held to precipitate fibrinogen and total globulin, respectively, at dilutions of 1:30. (No precipitate was obtained in the serum with 1.00 molar sodium sulfate, indicating an absence of euglobulin and fibrinogen.) Heparin was added in the concentration which gave the maximum turbidity at pH 5.0, in turbidity measurements such as Fischer has used, *i.e.*, 0.5 cc of a 1 per cent. solution of heparin was added to 0.5 cc of serum for each determination. No differences were obtained in the quantity of protein precipitated in serum or plasma in any concentration of salt.

[1] SCIENCE, 75: 443, 1932.
[2] A. Fischer, *Biochem. Z.*, 244: 464–485, 1932.
[3] Paul E. Howe, *Physiol. Rev.*, 5: 439–476, 1925.
[4] A. Fischer and A. Schmitz, *Naturwissenschaften*, 20: 471–2, 1932.

These results, while confirming Fischer's observations that heparin produces a combination in serum that is precipitated at approximately pH 5.0, does not substantiate his tentative conclusion that this combination is euglobulin or pseudoglobulin. Fischer's proofs of euglobulin have chiefly concerned procedures which depend upon precipitation at the isoelectric point; a sodium acetate buffer mixture, CO_2, dialysis. Precipitation near its isoelectric point is a characteristic of euglobulin; on the other hand, euglobulin, as it is ordinarily recognized, may be salted out with neutral salts. If the heparin-protein complex of new-born serum is euglobulin, it should have been precipitated at 1.00 molar sodium sulfate, or if only a partial combination the salting out might possibly have appeared at 1.50 volume-molar sodium sulfate. We realize that salting out is not an entire proof of a globulin. Salting out does, however, comprise one of the procedures used in differentiating and preparing euglobulin. Fischer's suggestion is exceedingly interesting, but its verification requires more evidence than isoelectric precipitation. It seems to us that this phenomenon observed by Fischer can be explained without assuming the actual formation of globulin.

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THE RELATION OF THE HYPOPHYSIS TO EXPERIMENTAL DIABETES

IT has been known since 1889 that removal of the pancreas leads to an increase in the blood sugar and appearance of sugar in the urine. Insulin, prepared from the pancreas, is effective in controlling the metabolism of sugar but does not cure the condition. Recently Houssay reported that extirpation of the hypophysis prior to removal of the pancreas was effective in preventing severe diabetes. It appears that we have confirmed this work. Two dogs did not survive long after the second operation, but in them the typical hyperglycemia did not develop. A third animal has survived over three weeks, during which time he has remained in good health. The tolerance for glucose is normal and the fasting blood sugar is within the normal range. No spontaneous glycosuria has occurred.

The animal shows certain symptoms which indicate complete removal of the hypophysis. Autopsy will show whether or not the hypophysectomy was complete and if there is accessory pancreatic tissue. This work is being continued at the University of Chicago.

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CANCER RESEARCH

IN the January, 1933, number of *The American Journal of Cancer* is an editorial on cancer research. In this the editor states that the prime needs of cancer research are first, *brains;* second, *time;* and third, *money.*

I should like to point out the obvious fallacy of any generalization of this type, even though it be made by one who for years has been connected with an institution utilizing each of the three components mentioned. In a field as complex as that of cancer research one institution may need primarily brains, another time for investigation and a third money. Furthermore, the primary need of any one institution engaged in cancer research may change from month to month or from year to year. It is also obviously true that no one component alone will result in progress. Brains without time or money result merely in theories. Time alone is obviously sterile. Money without brains or time is a material and impersonal factor. If each scientific man would avoid the field of generalization about the work of all others and would apply himself to his own problems in the way that he believes best, utilizing, in the proportions which he is able to find them and is able to develop them, the three elements of brains, time and money, more progress will result.

C. C. LITTLE

ROSCOE B. JACKSON MEMORIAL
LABORATORY
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REPORTS

APPROPRIATIONS FOR GRANTS IN AID BY THE NATIONAL RESEARCH COUNCIL

THE National Research Council wishes to announce that the research aid fund of which it has had charge for the past three years is to be continued during the present calendar year, 1933. The fund is administered by a special committee on grants in aid of research, which is composed of the chairman and treasurer of the council and of the chairmen of the council's seven divisions of science and technology. This committee will be ready to consider requests for grants of moderate amount from this fund for the support of the individual research work of qualified investigators in the fields of the natural sciences, who are citizens of the United States or of Canada.

Requests for grants from this fund will be acted

upon by the committee at two meetings during the year, in May and in December. It is requested that applications for grants to be considered at the meeting in May be filed with the council before March 15 and that applications to be considered at the December meeting of the committee be forwarded to reach the council before October 15, 1933.

The council has adopted the following policy for the administration of this research aid fund:

(1) Grants will be made in order to cover such expenses as the following: apparatus, materials and supplies, technical assistance, and field expenses.

(2) In general, grants will not be given for personal salaries or fellowship stipends, for expenses of publication, for the purchase of books, for travel to attend scientific meetings, or for research work of students under instruction.

(3) Preference will ordinarily be given to the support of investigations—(a) in which the problem and methods to be used are clearly stated, and in which definite results can be expected with the aid of the grant; (b) toward which the university or other institution to which the applicant is attached also contributes financially or through special support; (c) for which a grant of not more than $1,000 is requested.

(4) A report of progress should be made by the grantee to the Secretary of the Committee within a year after beginning the use of the grant.

(5) Periodical reports of expenditures from grants are expected, ordinarily, at the close of each quarter.

(6) The title to property purchased from grants will remain with the National Research Council until ultimate disposition of the property is made by the Council.

Correspondence in regard to applications for grants should be addressed to the Secretary, Committee on Grants in Aid, National Research Council, 2101 Constitution Avenue, Washington, D. C.

At its meeting in December, 1932, the committee on grants in aid made thirty-four grants for the support of individual research, as follows:

George E. Davis, Department of Physics, Iowa State College, development of improved methods of determining absorption spectra; Jacob Kunz, professor of theoretical physics, and J. T. Tykociner, research professor of electrical engineering, University of Illinois, determination of the magnetic moment of hydrogen; P. A. Ross, professor of physics, Stanford University, x-ray analysis and the Compton effect.

Charles B. Breed, professor of railway and highway transportation, Massachusetts Institute of Technology, cost of highway transportation; Wilber E. Harvey, assistant professor of metallurgical engineering, Lehigh University, combined effects of corrosion and fatigue upon welds; C. F. Hirshfeld, chief, research department, The Detroit Edison Company, in cooperation with H. Diederichs, professor of experimental engineering, Cornell University, determination of the temperature of the metal of a tube wall separating bodies of steam and water.

H. K. Benson, professor of chemical engineering, University of Washington, biochemical decomposition of sulfite waste; Cecil E. Boord, professor of organic chemistry, Ohio State University, synthesis of olefines; George A. Hulett, professor of physical chemistry, Princeton University, electrical conductivity of pure water; Wesley G. Leighton, assistant professor of chemistry, Pomona College, activation of surface phenomena in quartz by ultra-violet light; A. L. Robinson, assistant professor of chemistry, University of Pittsburgh, thermochemistry of dilute solutions; O. F. Stafford, professor of chemistry, University of Oregon, concentration of the H^2 isotope of hydrogen by the electrolysis of water.

Paul H. Dunn, fellow in geology, University of Chicago, correlation of Silurian strata in the Mississippi Basin; Ross L. Heaton, consulting geologist, Denver, Colorado, stratigraphy of the central and southern Rocky Mountain region.

Reginald D. Manwell, assistant professor of zoology, Syracuse University, effect of atebrine and rauwolfine on avian malaria; Eric Ponder, professor of general physiology, New York University, effects of intravascular lysins on red blood cells; Bret Ratner, clinical professor of pediatrics and lecturer on immunology, New York University and Bellevue Medical College, proteins and amino acids of milk; B. T. Simms, professor of veterinary medicine, Oregon State Agricultural College, salmon poisoning in dogs; Carl C. Speidel, professor of anatomy, University of Virginia, fundamental activities of nerves in the living animal; Maurice B. Visscher, professor of physiology, College of Medicine, University of Illinois, mechanical efficiency of heart muscle; Herbert U. Williams, professor of pathology, University of Buffalo School of Medicine, evidence of syphilis in mummified bodies from Peru.

William A. Cannon, lecturer in botany, Stanford University, derivation of oxygen by the roots of land plants; Henry J. Fry, professor of biology, New York University, mitosis in *Chaetopterus;* Ernst Gellhorn, professor of physiology, College of Medicine, University of Illinois, influence of metabolites and internal secretions on fatigue; Madeleine P. Grant, assistant professor of zoology, Smith College, thyroid secretion in amphibians and mammals; E. Raymond Hall, assistant professor of vertebrate zoology and curator of mammals, University of California, monographic study of the American weasels; Hans Jenny, assistant professor of soils, University of Missouri, ionic exchange of potassium on soil colloids; Harold Kirby, Jr., associate professor of zoology, University of California, flagellates in termites; C. R. Orton, professor of plant pathology, West Virginia University, natural dissociation of *Fusaria* in the soil.

C. W. Brown, professor of psychology, University of California, central nervous mechanism of emotional responses; Clarence H. Graham, assistant professor of psychology, Clark University, analysis of visual response by means of an electrical recording method; Harry F. Harlow, assistant professor of psychology, University of Wis-

consin, effect of complete striate muscle paralysis upon learning and thinking; Truman Michelson, ethnologist, Bureau of American Ethnology, Smithsonian Institution, study of special data upon the ethnology of the Fox and Ojibwa Indians; William S. Webb, professor of anthropology and archeology, University of Kentucky, archeological survey of Kentucky.

W. H. HOWELL,
Chairman, National Research Council

SCIENTIFIC APPARATUS AND LABORATORY METHODS

A METHOD OF OUTLINING CUTANEOUS NERVE AREAS

IN 1928 we discovered that the cutaneous area supplied by a nerve may be rendered insensible to light touch by subjecting the nerve trunk to the influence of an alternating current; the area may be outlined by the procedure employed in cases of peripheral nerve lesions. Though we have not explored the literature to its depths, our efforts in this direction have failed to reveal a previous record of such a finding.

This indicated a new experimental approach to some of the problems of cutaneous innervation and sensation; such work has been pursued in this laboratory since 1928, and preliminary reports have appeared.[1] We did not intend to publish the technique apart from the results, but requests for an account of the method indicate that a brief note would place it at the service of others who might find it useful. We shall describe the apparatus used by us, suggesting some desirable modifications; others will occur to those who use the method.

The current from the 110 volt D. C. main was converted into an alternating current by a small Alexanderson alternator (Fig. 1); because of its noise this should be in another room than that wherein the observations are being made. The alternator may be driven by A. C. or by D. C.; Fig. 1 shows the latter arrangement. The frequency was controlled by a variable resistance in the circuit driving the alternator; it was estimated from the number of teeth and the r. p. m. of the alternator, this being ascertained by a Starrett speed indicator. The current strength was controlled by a system of variable resistances in the primary or input circuit (Fig. 1); our method of measuring it was unsatisfactory and need not be described; that this is unimportant is indicated below.

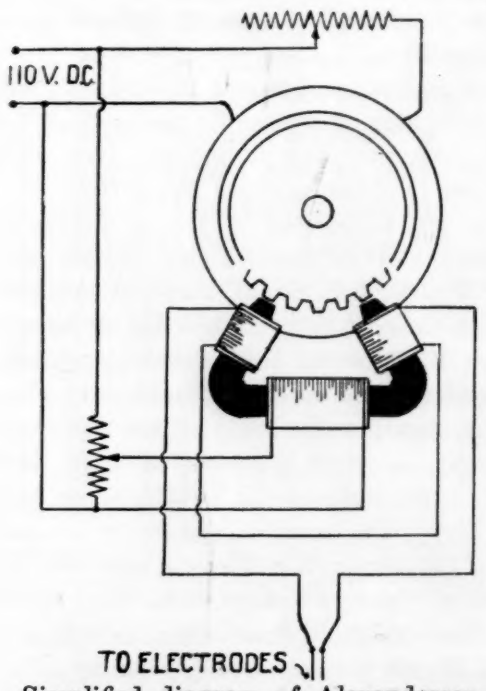

FIG. 1. Simplified diagram of Alexanderson alternator, with driving circuit (above), input circuit (left), and output or stimulating circuit (below).

We have applied the method systematically only to the nerves of the forearm and hand, to which the following account refers. Each nerve to be investigated was subjected to unipolar stimulation through the intact skin. We used metallic electrodes, padded with gauze soaked in a saturated solution of NaCl; subsequently we have found a 4 per cent. solution equally satisfactory. The large inactive electrode was applied to the opposite arm. The stimulating electrode (diameter about 1 cm) being placed on the skin over an appropriate part of the nerve trunk, somewhat as indicated by Hughson,[2] the current was *gradually* turned on by withdrawing resistance from the input circuit. With practice the nerve was soon "picked up"; this was evidenced by the sensation of flutter projected into its cutaneous area, as described by Hughson.[2] As the current was increased, testing with a von Frey hair revealed that at a certain level of current strength tactile sensibility disappeared in the area supplied by the nerve under stimulation, though remaining unaltered elsewhere; to this loss of sensation we apply the provisional term "masking" in preference to anesthesia. Manipulation of the electrode and of the current extended the masked area to a limit beyond which no further manipulation could extend it; it was then outlined on the skin as in the case of a peripheral nerve lesion, every precaution being taken to ensure that the subject received only tactile stimuli.

Fig. 1 shows no apparatus for measuring the frequency and strength of the current, that being unnecessary if one is merely outlining nerve areas: it is sufficient to manipulate the current (and the electrode) until the area is masked. If he so desire, the

[1] I. M. Thompson, V. T. Inman and B. Brownfield, *Anat. Rec.*, 45: 245, 1930; I. M. Thompson and A. Barron, *ibid.*, 48: 35 (Suppl.), 1931; I. M. Thompson, *Jour. Anat.*, 66: 148, 1931.

[2] W. Hughson, *Anat. Rec.*, 23: 371, 1922; *Johns Hopkins Hosp. Bull.*, 33: 338, 1922.

investigator may introduce suitable meters, etc., at appropriate places in the output or stimulating circuit. As a result of further work, we recommend a larger generator than that described above, yielding stronger currents at low frequencies. For the subsequent development of this field of investigation such a generator has been designed and installed in this laboratory, and will be described later; using it, satisfactory masking occurs with currents of about 100 to 300 cycles per second, 5 to 10 volts and 0.5 to 3 milliamperes. All our work on nerve areas, however, was done with the small alternator. Possibly currents generated otherwise may yield similar results.

The space available here precludes reference to the difficulties inherent in such a procedure; these will be discussed in a more detailed publication. The method is not easy, and demands practice and patience on the part of observer and subject alike. We have encountered one or two subjects who, for some obscure reason, possibly physiological or psychological, were unsatisfactory for such work, wherein the importance of reliable subjective responses is obvious. Outlining the same area on the same subject on different occasions has yielded reasonably consistent results.

The nature of the results is indicated in Fig. 2.

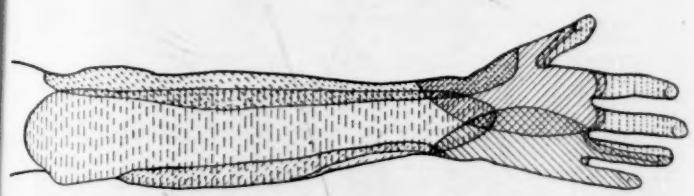

FIG. 2. A sketch of the dorsal aspect of the right forearm and hand of an adult male, showing the cutaneous areas supplied by various nerves; these are not labelled, for anatomists will recognize them. In this individual the median nerve supplied the entire dorsal aspect of the index and middle fingers. The extent of the overlaps is easily seen. In this person certain small areas on the back of the wrist were supplied by three cutaneous nerves: for example, that in the middle is seen to be supplied by the dorsal cutaneous nerve of the forearm, the radial and the ulnar.

So far as we know, ours are the first pictures of the *complete* innervation, *including overlap areas,* of any region of the body in single individuals. We have succeeded in estimating the size of these areas and in studying their variations; preliminary results have been presented by Thompson, Inman and Brownfield, and by Thompson[1]; a detailed analysis is being prepared.

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CELLOPHANE ROLL FILMS AND THE FIXING OF CARBON PAPER TYPING ON CELLOPHANE[1]

THE wide-spread interest in the use of cellophane for slide lanterns[2] seems to justify reference to the paper presented by the authors[3] before the Division of Chemical Education of the American Chemical Society, at Denver, Colorado, on August 24, 1932.

A cellophane roll film carrier was described for using the cellophane in rolls instead of slides. The device is fitted into any ordinary slide lantern with no modification of the lantern necessary. Any one interested in this device will find a scale drawing for one form of it given in the *Journal of Chemical Education.*[3] The cellophane roll film carrier will hold over 33 feet of 0.001 inch thick cellophane. This is equivalent to about 120 slides, yet forms a roll less than ¾ inch in diameter, and weighs less than one glass slide. The article also gives a description of methods of preparing the films, including colored pen work; also uses of such films, and methods of fixing carbon paper typing.

Typing on cellophane slides and films is done with carbon paper. Every one seems to have had trouble with smearing of such typing, but we have worked out a simple method which is so satisfactory that a roll film has been used over 400 times and still shows no evidence of smearing. A brief description of it may be of interest to readers of SCIENCE.

The procedure consists of passing the typed film through a suitable liquid and carefully blotting it between unglazed paper while it is still moist, care being taken that the blotting paper does not slide over the wet surface. The paper removes most of the liquid and the excess ink. The film is then placed between dry paper and pressed for several minutes to complete the fixing.

Of fifty chemicals tried, organic liquids for the most part, the most satisfactory for the purpose are: amyl acetate, ethyl acetate, methyl acetate, acetaldehyde, benzyl alcohol, iso-amyl alcohol, ethylene glycol monoethyl ether ("cellosolve") and a mixture of 75 per cent. di-ethyl ether and 25 per cent. ethyl alcohol.

Other liquids fairly satisfactory are: iso-butyl alcohol, iso-propyl alcohol, methyl-ethyl ketone, acetone, ethylene chloride, ethylene bromide, benzyl

[1] Contribution No. 89 of the Division of Industrial Sciences of West Virginia University.

[2] "Lantern Slides from Cellophane," John L. Wilson, *Jour. Chem. Educ.*, 8: 2212 (November, 1931); "A New Use for Cellophane," K. L. Warren, SCIENCE, 76: 573, (December 16, 1932); "Cellophane for Slide Lanterns," B. H. Walden, SCIENCE, 77: 91 (January 20, 1933); "The Radio-Mat," F. L. Wells, *ibid.*

[3] "Cellophane Roll Films for Slide Lanterns" was sent to the *Journal of Chemical Education* on July 20, 1932, and appears in a somewhat abbreviated form in *Jour. Chem. Educ.*, 10: 92–94 (February, 1933). Since August numerous brief references to the paper have appeared in the daily press and periodicals.

chloride, nitrobenzene, di-ethyl aniline, pyridine, and carbon tetrachloride.

Results are much better when the chemicals of the first list are used.

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SILK CELLOPHANE FOR LANTERN SLIDES

RECENTLY Warren,[1] Walden,[2] and Wells[3] suggested the use of plain cellophane as a recipient of carbon in projection lantern slides. As a further suggestion, special du Pont Number 300 white silk cellophane takes ink directly from the typewriter ribbon without smudging and, after momentary drying, the record is permanent. If the original impressions are gone over for the second typing, legibility is enhanced. The cost of this special silk cellophane is less than one cent per slide.

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SPECIAL ARTICLES

THE RELATIONSHIP OF BACTERIUM GRANULOSIS TO TRACHOMA

THE question of the relationship of *Bacterium granulosis* to the etiology of trachoma has been discussed in a recent publication.[1] As stated in that article the organism of Noguchi "merits consideration as the etiological factor" in the disease. This view was expressed in spite of the accumulation of negative evidence of many workers, including the writer, and the doubts which have been expressed by some who have been the most sanguine in their expectation of the solution of the trachoma problem following the isolation of this organism with which a transmissible granular condition may be produced in monkeys. Such doubts are based on the failure of many investigators to isolate the organism from trachoma in different parts of the world and the considerable number of negative results which have been obtained in attempting to produce trachoma by inoculation of human subjects with the organism.

I reported that the granular condition originally induced with difficulty in *Macacus rhesus* monkeys by inoculation with cultures of *Bact. granulosis* was very readily transmissible and that transmission could be accomplished by merely rubbing a sterile swab over the affected conjunctiva and then rubbing it over the conjunctiva of a normal animal, thus demonstrating the fact that it is not necessary to excise tissue and to inject this subconjunctivally. In other words, as described by one worker, the granular condition may be described as one which is "highly infectious."

The question arose: "Is human trachoma as readily transmissible?" There are a number of clinicians who, after long experience with trachoma, still question the ready communicability of the disease. On the other hand, Taborisky[2] inoculated the conjunctiva of 5 blind subjects with the conjunctival secretion of trachoma cases and all acquired the disease.

In order to obtain a comparison between the granular condition induced in monkeys by inoculation with cultures of *Bact. granulosis* and that induced by direct transfer of secretions from trachoma cases, a series of monkeys was started in the early part of 1932 in which granular lesions were produced by repeated swabbing of secretions from trachoma cases in Rolla, Missouri. A number of attempts had previously been made to accomplish this without success. A granular condition which developed slowly was obtained in 2 monkeys and from one of these was transmitted to another monkey by repeated swabbing. As reported recently[3] two parallel series of monkeys (8 in each series) were then considered, one in which attempts were made to transmit the granular condition originally induced by inoculation with cultures of *Bact. granulosis* and the other in which attempts were made to transmit the granular condition originally induced by transfer from trachoma cases. Four monkeys in each series had been previously inoculated with a vaccine of *Bact. granulosis* with the idea that the test might show whether there were immunological differences in the two conditions. In the "culture" series 5 of the 8 animals developed the granular condition after one swabbing from an infected monkey, and one after two swabbings, and one died. In all these the granular condition occurred spontaneously in the uninoculated eye. In the "direct transfer" series all the monkeys were swabbed 3 times (on consecutive days) and one of the 8 developed a granular condition in both eyes, 6 remained unaffected and one died. Of the 6 unaffected, 5 have since been swabbed from infected monkeys, one a single time, one 2

[1] K. L. Warren, SCIENCE, 76: 573, December 16, 1932.
[2] B. H. Walden, SCIENCE, 77: 91, January 20, 1933.
[3] F. L. Wells, SCIENCE, 77: 91, January 20, 1933.
[1] Ida A. Bengtson, *Pub. Health Rep.*, 47: 1914, September 19, 1932.
[2] J. Taborisky, *Graefe's Arch. f. Ophth.*, 123: 140, 1930.
[3] Ida A. Bengtson, *Pub. Health Rep.*, 47: 2281, December 9, 1932.

times and three 3 times (on consecutive days). In 4 of these lesions have developed gradually and in one of these the uninoculated eye has become affected. In neither series was protection afforded by the vaccine, since more were affected in the vaccinated than in the unvaccinated groups. Regarding the gross appearance of the lesions there was no great difference. The condition was rather less active in the "direct transfer" series than in the "culture" series. The results obtained indicate that the condition induced by direct transfer from trachoma in Missouri was less easily transmissible than that induced by inoculation of cultures of *Bact. granulosis*. Quoting from the report "Whether this difference is significant and whether it would constantly be true can not be said without further tests." It may be that as in other diseases certain strains of the organism concerned may be more virulent than others.

The results reported by Olitsky, Syverton and Tyler in SCIENCE, for January 6, 1933,[4] lend support to the view that *Bact. granulosis* is concerned in the etiology of trachoma. Of significance is the use of tarsectomized tissue in the experimental work carried out by these investigators. The chances for successful transmission of the condition to animals and for isolation of the organism concerned are without doubt greatly enhanced by the use of large amounts of material. The great amount of negative evidence in certain localities as reported in the literature and in unpublished work concerning which information has been received by the writer, may possibly be explained on this basis. It is apparent that most workers have not used tarsectomized tissue, which is difficult to obtain because many ophthalmologists consider the tarsectomy operation as of questionable value for the cure of trachoma.

Another phase of the problem is of interest. If the work of Olitsky and collaborators can be confirmed in certain other parts of the world where only negative results have been reported, using the sort of material and the methods employed by them and it is definitely established that *Bact. granulosis* is the etiological factor in trachoma, then the isolation of an organism which is non-filtrable and which grows on the ordinary culture media, from a disease which is characterized by the presence of inclusion bodies, takes on a certain significance. A revision of the rather generally accepted view of the nature of the infective agent in at least some of the group of diseases in which inclusion bodies occur (diseases usually classified as belonging in the "filtrable virus" group) would be necessary. As I reported in 1928[5] in a study of inclusion bodies in over 200 cases of trachoma these bodies were present in nearly 50 per cent. of the cases. More recently, Taborisky[6] has reported that observations during a period of 20 years justify the belief that there can be no trachoma without inclusion bodies at some period, if cases are followed from the beginning. As the result of the microscopical study I reported that the inclusion body in the early stages was composed of rod-shaped organisms. A photograph of a very unusual preparation is shown in this publication, in which the nucleus of an epithelial cell is surrounded by numerous rod-shaped organisms which morphologically at least could very well be said to correspond with *Bact. granulosis*, though identification on this basis is obviously impossible. The definite rod-shaped forms are seen very rarely. Usually the inclusion body appears as a more or less structureless mass, in which at a later stage very minute coccoid bodies staining reddish with Giemsa appear. Apparently the group of organisms forming the inclusion body is acted on by the living cytoplasm of the epithelial cell and transformed into these small bodies which are the "elementary bodies" of Halberstaedter and von Prowazek. These are visible when occurring in the cell, but it may be that they occur outside the cells also, in which case they are indistinguishable or very nearly so. The difficulty of cultivating such forms can be readily understood. As stated in an article now in press, these "elementary bodies" are probably for the most part non-cultivable and they may constitute the active infectious agent, the so-called "virus," while the rod forms which are cultivable occur only rarely. In a word the organism when developing in the tissues may occur in a different form than when growing on artificial culture media. The supposition offers an explanation of the difficulty of cultivating the organism unless large amounts of material are used, in which the chances of encountering the definite rod forms are increased.

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U. S. PUBLIC HEALTH SERVICE

[4] P. K. Olitsky, J. T. Syverton and J. R. Tyler, SCIENCE, 77: 24, January 6, 1933.

[5] Ida A. Bengtson, *Pub. Health Rep.*, 43: 2210, 1928; *Amer. Jour. Ophth.*, 12: 637, 1929.

THE ACTION-CURRENT AS MEASURE OF MUSCLE CONTRACTION

IT has been considered impossible to show a definite quantitative variation of the action-current with the contraction of human muscle. Upon contraction the oscillations of the oscillographic curve, which are always running even with resting muscle, coarsen and widen, but it is impossible to correlate changes of frequency or changes of amplitude with the degree of contraction, for the good reason that the oscillations during the contraction have no definite frequency and no definite amplitude; detailed study of the curve

[6] J. Taborisky, *Graefe's Arch. f. Ophth.*, 124: 453, 1930.

shows that the oscillations are extremely irregular. A significant measure of the action-current during contraction must be in terms of a summation of the current.

The planimeter measure of the area generated by the oscillographic curve during the contraction-disturbance is possible; but the method is tedious and inaccurate. But the summation of the action-current disturbances by a ballistic galvanometer is convenient and accurate; the action-currents have only to be amplified without distortion, rectified and photographically recorded.

An adequate method of measuring the action-currents from the muscle contraction is the less difficult part of the problem. There must also be some measure of the force of the contracting muscle. The group of muscle fibers must be isolated so that the contraction has a definite beginning and end in time; and the force that they exert must be measured in terms of some simple physical result. If the contraction is opposed by antagonistic muscles excess heat will be developed which it is impossible to measure.

The type of movement known as "ballistic," and familiar in many forms of skilled movement, like piano-playing and typing, can be made to fill these conditions. In the free (or "loose") movement of a skilled pianist, the flexor group actuates the forearm by a sudden pulse which has a uniform duration of about 45 sig.; it is quite unopposed by the extensors and sets the limb in motion with a very rapid acceleration, whereafter for three fourths of the stroke the limb swings free by momentum with a uniform velocity which is easily measurable; under proper conditions the length of the stroke varies with the velocity. It is easy to check such movements from the kymograph tracing; during the momentum phase the tracing is a right line which can easily be tested with a straight-edge; in a series of such strokes the ratio of the velocity to the length of stroke is fixed if they are ballistic. A skilled pianist can make such free and ballistic movements eight out of ten times.

Since the mass of the moving limb is constant and the path of the excursion can be made constant, the velocity of the ballistic movements becomes a measure of the varying force of the contractions. For the success of the study we found the choice of skilled pianists as subjects a vital point. The upper-arm was fixated so that the elbow joint was maintained in the same position to make the path of the excursion constant. The forearm was carefully supported so that a free, horizontal movement could be repeated without any influence of gravity, and the movements were recorded on a kymograph.

Electrodes of Ag or Zn shaped to fit the surface, wrapped in muslin wet with NaCl solution, were bound firmly with adhesive tape to the arm over the respective muscles. Under these conditions a series of movements were recorded and the corresponding contractions were registered from the ballistic galvanometer.

An actual series of strokes gave the ratios of excursion to velocity: .28, .26, .31, .31, *.23, .24,* .31, .28, *.35,* .26, .32. It is apparent that the fifth, sixth and tenth strokes are not free; and a check of the kymograph tracing with straight-edge confirms this. If the summated action-currents vary with the velocity, the ratio of velocity to action current should be fixed; the actual values in this case were: 1.0, 1.2, 1.1, 1.2, *1.7, 2.0,* 1.4, 1.2, *0.9,* 1.1, 1.1. With the exception of the tense strokes, fifth, sixth, tenth, the values vary within 10 per cent., which is significant in physiological measurement.

When movements of this type occur against a fixed external tension, it was found possible to fit the results to an equation in which a constant (a) multiplied by the excursion plus a constant (b) is equal to the action-current (a Exc. + b = Ac. cur.). This indicates that a constant number of fibers is employed in overcoming the fixed resistance.

If a to-and-fro movement of this form is repeated in series in which both the beat-stroke and back-stroke are ballistic, the results are much more precise. A series of 25 movements with 3-inch excursion, a series of 25 with 5-inch excursion and a series of 25 with 7-inch excursion, all over the same path and at the same rate per sec., were recorded on the kymograph. In such series all the force exerted by the contraction of the flexor group of fibers and of the extensor group of fibers appears in the momentum of the to-and-fro movements; and the momentum varies directly with the length of excursion.

Parallel records were made by a ballistic galvanometer of the summated action-currents of the flexors and by a second ballistic galvanometer of the summated action-currents of the extensors. The sum of the summated flexor- and extensor-action-currents should be proportional to the length of excursion of the respective series of movements (which is an indicator of the force exerted). Thus in an actual case, the readings show a series of 2-in. excursion with a ratio to the sum of the action-currents of .86, and a series of 5-in. with a ratio of .86, and a series of 7-in. with a ratio of .83. When repeated in reverse order, the series give ratios: .83, .86, .85.

It is easy to make an ocular demonstration of this quantitative relation between action-current and muscle contraction by sending the amplified, rectified action-currents from rapid to-and-fro movements through a milliameter or a level indicator; the oscillating pointer will indicate a middle point for the series of contractions from a series of horizontal ballistic

movements. The one important thing is to have a skilled pianist as subject capable of making the free ballistic movements. If the rate per sec. of the movements, as well as the path, is fixed, it is easy to see the shift of this middle point of oscillation of the indicator pointer with each change in length of excursion of the movement series, and approximate readings may be made to show the proportionality.

If the subject increases the rate per sec. of such a series of ballistic movements, the readings of the level indicator show some interesting relations. As the subject approaches his maximum rate, the amount of action-current indicated reaches a maximum; apparently the muscles are exerting their maximum force; from that stage on the increase in rate per sec. reduces the excursion proportionally. The maximum rate per sec. is achieved with the minimum excursion; in the final stage the excursion is very slight and the coordination is irregular and a "forced tremor" supervenes.

It is evident that there is a maximum number of available fibers for a given movement and when that number is in action the increase of rate of the repeated movement depends on reducing the excursion so that the flexor and extensor groups of fibers can each take up the momentum in reversing the movement of the limb; at last the coordination breaks down, the movements are irregular and a forced tremor is the result.

These results were obtained repeatedly with some four subjects. The maximum rate differs somewhat from subject to subject, but there are no essential differences in the quantitative relations of action-current and contraction. It is obvious that the findings fit the all-or-none hypothesis for the contraction of human muscle fibers.

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CHEMICAL VERSUS MORPHOLOGICAL SPECIES DIFFERENCES

THE writer has had the opportunity to make analyses of the sap of plants of the genus *Valonia* from many localities. Under current classification based upon morphology, these plants are referable to three or four different species. But, as has already been pointed out in the case of plants classed as *Halicystis ovalis* (Lyng.) Areschoug, chemical differences contravene morphological characteristics.[1] These chemical distinctions have since been utilized in segregating part of the genus *Halicystis* as a new species.[2]

Table I shows the relevant data in the case of genus *Valonia*. The new analyses in this table were made by the following methods: Halide was determined by titration with silver nitrate; it consists predominantly of chloride. K + Na was determined as total sulfate, K as chlorplatinate and Na by dif-

TABLE I

CHEMICAL CHARACTERISTICS AND ENVIRONMENTAL DATA FOR VARIOUS REPUTED SPECIES OF VALONIA

Reputed species	Locality	Habitat	Depth	Water temperatures*	Composition of sap				Condition of materials	Authority (See footnotes)
					K	Na	Cl	K/Na		
			m	°C.						
Valonia utricularis	Naples	On rocks	0	?	368	20.4	369	(18.0)	= –	4
	"	" "	0	?	291	152	418	1.91	= –	5
	"	" "	0	18°	372	266	639	1.43	Fair (March)	–
V. macrophysa	Naples	On tunicates	30	12°	465	164	657	2.77	Good (February)	–
	Dry Tortugas, Fla.	On masonry	0	28°	494	97	617	5.10	Good (June)	6
	Bermuda	On coral rocks	0	25°	517	90	597	5.73	–	7
V. ventricosa	Makahaa, Tonga	Under rocks	0.3	25°	497	125	619	3.97	Excellent (Sept.)	–
	Main shore, Tonga	On sea weeds	1.0	25°	545	55	620	9.92	" "	–
	Dry Tortugas, Fla.	Under rocks	0.3	28°	562	46	618	12.21	" (June)	–

* Water temperatures are only rough approximations.
+ Mean of four controls.

[1] S. C. Brooks, *Proc. Soc. Exp. Biol. Med.*, 27: 409–12, 1930.

[2] L. R. Blinks and A. H. Blinks, *Bull. Torrey Bot. Club*, 57: 389–95, 1930.

[3] A. Meyer, *Ber. Deutsche Bot. Ges.*, 9: 77–9, 1891.

[4] S. Camlong and L. Genevois, *Bull. Sta. Biol. Arcachon.*, 27: 309–21, 1930.

[5] E. Pantanelli, *Bull. Orto Bot. R. Univ. Napoli*, 6: 1–37, 1918.

[6] S. C. Brooks, *Protoplasma*, 8: 389–412, 1929.

[7] W. J. V. Osterhout, *Jour. Gen. Physiol.*, 5: 225–30, 1922.

ference. Data as to habitat, depth, and water temperature are germane, because of their possible physiological effects on different representatives of a single species. All plants were freshly gathered, and in good condition, except as noted.

The data relating to *Valonia utricularis* show that in this species there is relatively little selective accumulation of potassium: this species stands at the bottom of the list in so far as selective permeability is concerned. Meyer[3] regards his data as probably inaccurate, and this is almost surely the case with regard to the value for Na. All three analyses refer to plants from the same vicinity, and some seasonal differences are to be expected, as Camlong and Genevois have shown in the case of other marine algae.[4]

Plants usually referred to *V. macrophysa* Kütz have been collected for analysis from three stations: Naples, Bermuda and the Dry Tortugas Keys of Florida. The plants from the Golfo di Pozzuoli near Naples, where they grow at a depth of about 30 m, attached to tunicates, show less power of selective accumulation than do plants supposedly of the same species, which grow attached to rocks in shallow water in Bermuda and Florida. The former grew at a temperature which at the time of collection (January) was not far from 12°, while the latter were flourishing at a water temperature of about 28°. Some suggestion of a morphological difference is apparent, although Professor W. R. Taylor has been unable after careful study to find any certain difference. Yet in Florida the ratio of K to Na is about 5, while near Naples it is about 2.75. Other collections at Naples showed some variation from this figure, but the ratio of K to Na did not in any case exceed 3.17. The Bermuda plants resemble the Florida plants in all respects.

Are we here dealing with physiological variants, or with distinct valid species? If the habitats of the two types could be interchanged would their physico-chemical characteristics change too!

The plants classed in the table as *V. ventricosa* present a still more puzzling situation. The two samples from Tonga tentatively assigned to this species may very well in one of the two cases be *V. forbesii* J. G. Aghard. Morphological characters said to distinguish the two species are most unsatisfactory, and field identification was impossible. Professor W. A. Setchell has kindly examined specimens of the Makahaa collection, and tentatively assigns them to *V. ventricosa*. The habitat of the plants collected at Makahaa was like that of *V. ventricosa* in the Dry Tortugas of Florida: under coral rocks on sand in about one foot of water or less at low tide. The plants along the main shore reef of Tonga grew on and among other algae in considerably deeper water. Yet the sap of these latter plants was like that of Florida specimens of *V. ventricosa*, while that of the former was much more like that of *V. macrophysa* Kütz from Florida and Bermuda.

These facts lead us to inquire whether physico-chemical characteristics which are quantitatively well defined may not be valuable aids to the systematist. In some cases it is possible that environmental factors alone have produced the chemical differences noted. Yet it hardly seems likely that this is ordinarily the true explanation, any more than that it is the true explanation of morphological differences. Further study may well make chemical analysis an indispensable tool for the systematist.

My sincere thanks for their courteous generosity are due to the director and staff of the Stazione Zoologica at Naples, where part of this work was done, and to Columbia University and the Woods Hole Marine Biological Laboratory for granting me the use of the table supported by them. Different phases of the work were made possible by grants from the National Research Council, the Bache Fund of the National Academy of Sciences, which made it possible for Mrs. M. M. Brooks to collect samples for me at Tonga, and from the Board of Research of the University of California. For all these I wish to express my thanks.

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